LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Thickness of the Earth's Crust

UPON my return to London yesterday, I received the two last numbers of NATURE (May 11 and 18), in both of which I find communications on this subject. In the first of these, by Archdeacon Pratt, that gentleman inserts a quotation from a lecture delivered by me, on January 29, this year, "On the Nature of the Earth's Interior" (*vide* NATURE, February 9, 1871), to the effect that the recent experimental researches of the eminent astronomer and mathematician, M. Delaunay, had destroyed the basis upon which the late Mr. Hopkins's reasonings, as to the solidity of the earth's interior, were founded, and asks the lecturer, *i.e.*, me, "I wonder why he has taken no notice of my letter in reply to M. Delaunay, which was printed in your journal for July 1870, six months before the lecture was delivered, and which also appeared about the same time in the *Philosophical Magazine* and the *Geological Magazine.* In this I showed that M. Delaunay had evidently misconceived the problem, and that Mr. Hopkins's method is altogether unaffected by his remarks."

As Archdeacon Pratt has the candour to admit that "any one with an ordinary degree of knowledge of popular astronomy and of mechanical action is quite competent to form a good opinion on the point in dispute," I would, in answer to the question he puts to me, simply state that, after a careful study of the letter he refers to, upon its first appearance in the *Philosophical Magazine,* I *purposely* avoided referring to it in my lecture, since I failed to discover that the author had in it "showed that M. Delaunay had evidently misconceived the problem," or any reasons whatsoever which could shake my faith in the conclusions of M. Delaunay, subsequently confirmed experimentally by M. Champagneur. I would also mention that, previous to this lecture, I attended the meeting of the Royal Society on the 22nd December, 1870, expressly to hear a subsequent paper by Archdeacon Pratt "On the Constitution of the Solid Crust of the Earth," on which occasion the opinions of Professor Stokes and the experimental demonstration of Mr. Siemens, as to the untenable nature of the author's conclusions, still further confirmed me in the views I put forth subsequently in my lecture.

It is now superfluous to specify in detail the precise reasons for my rejecting the arguments of Archdeacon Pratt, as I have, in a great measure at least, been anticipated in so doing by the substance of two letters, signed respectively "A. J. M." and "A. H. Green," which appeared in my absence in the last number of NATURE; to these I may refer in support of my view, in which I may also add one of our first English mathematicians has concurred; that M. Delaunay has not changed his will be seen from the Proceedings of the Academy of Sciences at Paris, March 6, 1871.

Having always entertained the highest opinion of the scientific labours of the late Mr. Hopkins, I have taken pains to make myself acquainted with his writings as far as possible for me; but when Archdeacon Pratt states "what Mr. Hopkins did may be divided into two parts—he first *conceived an idea,* which was to be the basis of his calculation; and then he made the calculation," I regard the whole pith of the question as embodied in these words, which admit that Mr. Hopkins based his elaborate calculations upon *an idea,* now shown by M. Delaunay to be incorrect, whilst the latter gentleman, on the contrary, founds his deductions upon premises which he first proves to have stood the test of experiment. Where eminent scientific men are arrayed on each side of a question of this nature, the remarks made in the last paragraph of the archdeacon's communication seem rather out of place, and might be applied with equal force in an entirely opposite sense to that intended by their author.

May 20 DAVID FORBES

The Geographical Distribution of Insects

IN NATURE (No. 74, p. 435) was a very interesting article on geographical distribution by Mr. Wallace, combating some recently-urged views of Mr. Murray's. Mr. Wallace took, as an example, the Madeira Islands, and sustained his position upon the numerical statistics furnished by Mr. Wollaston in his books. That these conclusions are very different from those arrived at by Mr. Wollaston is evident and as a six months' residence in the more remote group of the Canary Islands confirmed to my mind Mr. Wollaston's position, while bringing into relief facts utterly incompatible with Mr. Wallace's, I have ventured to publish a few remarks on the question.

Mr. Murray's views of the distribution of beetles seem to me resolvable into saying that there are two faunas, a tropical (Brazilian and Africo-Indian) and an extra-tropical one. My own slight researches in exotic coleoptera (confined hitherto to the Coccinellidæ) strongly confirm this; and a curious instance of the connection between the northern and southern extra-tropical faunas occurred to me the other day. *Eriopis connexa,* a rather pretty little ladybird, occurs from Hudson's Bay and Vancouver's Island all the way to the Straits of Magellan; following, of course, the line of the Andes. But my object was principally to question some of Mr. Wallace's conclusions with regard to the Madeiran fauna. First of all, I was struck by the absence of any hypothesis for the origin of the very curious endemic forms which form the most important part of the fauna, and which most closely unite it to that of the Canaries and Azores. These Mr. Wollaston, myself, and apparently Mr. Murray regard as affording proof that these islands, or rather groups of islands, were once parts of a considerable continent, and I certainly am at a loss to see how else they are to be explained; for though Mr. Wallace regards the Madeira islets as possibly formerly connected, he would, I suppose, be unwilling to extend this to the other groups. Mr. Wallace appears to regard Mr. Murray's hypothesis to be that the Atlantic continent, of which Madeira is a remnant, derived its fauna from Europe; but it seems rather to be that in the Miocene period (or earlier) there was a similar continent, connected indeed with Europe, not deriving its fauna from Europe any more than Europe from it. Perhaps the best way of answering Mr. Wallace's view will be to take the case of the Canary Islands, whose fauna, resembling the Madeira as it does so closely, must have had a similar origin. Here the argument from apterous genera fails to a very great extent. Thus *Carabus* is represented by three species, while in S. Spain there is one, and in N. Africa only six or seven. *Thorictus* has three representatives, and here it may be noticed that ants'-nest beetles are decidedly not numerous in the islands, so that the "unusual means of distribution" fail on the whole to get them across the water. *Rhizotrogus* is represented by the closely allied also N. African genus *Pachydema.* Of the very numerous European *Rhizotrogi* only two Sicilian ones are apterous, so that its absence in Madeira tells either way. *Otiorhynchus* is no doubt absent, but its place is more than supplied by *Atlantis* (20 sp.) and *Laparocerus* (30 sp.). *Pimelia* again is represented in the Canaries by twelve species, and the apterous genera of Heteromera by more than fifty species, which almost demonstrates the necessity of looking for Tenebrionidæ in localities where they are likely to occur.

Tarphius it certainly is difficult to conceive carried across by winds or waves, seeing that its habits are so retired that it has escaped notice till very recently in Europe. Now, however, it is beginning to turn up in suitable mountain localities of Andalusia, Portugal, the Apennines, Sicily, and Algeria; four species are described, and I have seen two others, all agreeing *inter se* and differing in structure from any Atlantic species. Moreover, it must have been carried apparently to the Azores as well. Then of the peculiar apterous genera quoted, *Thalassophilus, Torneuma, Scoliocerus, Xenomma,* and *Mecognathus* occur now also in Europe, requiring only a collecting-power equal to that of Mr. Wollaston for their discovery. There remain as puzzles upon the hurricane theory twenty-two blind species in the Madeira and the Canaries, and the whole series of Euphorbia-infesting species, fifty in number, all winged, and forming for the most part special genera. Finally, with regard to the fauna of the Azores, the condition of the islands must be taken into account; if the species found round Santa Cruz, Oratava, and Funchal were enumerated, about this proportion of European species would be found. The best island, Pico, has not been worked, and in the others almost all the original vegetation has disappeared. The fact that in the scraps (as they literally were) of Euphorbias, *Tarphius* and *Acalles* occurred, shows that if any of the pristine flora could be found a fair number of species might be expected. *Elastrus dolosus* may certainly have come from Madagascar by the very ingenious route sketched out by Mr. Wallace; but the occurrence of *Urania* in Madagascar, Brazil, and the West Indies suggests a possibly shorter route, even though no *Elastrus* be known as yet to occur in America.

In conclusion I may state that I am going to spend a year or

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perhaps two in the West Indian Islands, and hope there still further to investigate the theories of geographical distribution, especially endeavouring to see if they can in any way be regarded as having been connected with this submerged continent of Atlantis.

G. R. Crotch

The Coronal Rifts

Will Captain Tupman kindly explain what he means by "*actinic rifts*"? I should have supposed that the rifts are evidence of the absense of actinism at the places where they occur.

I am not at all surprised that anyone observing through a telescope should fail to notice the rifts. The eye would naturally be attracted by the bright light of the corona and the red prominences. It may be observed, also, that there is in the photographs a considerable amount of bright corona at the places where the rifts occur, so that Captain Tupman might have had the telescope pointed at the very places where the rifts were, and yet they would escape his notice. The rifts are *there*, unquestionably, in the two photographs taken (after the lapse of nearly an hour) at Cadiz and Syracuse, and the sketches taken in Spain also show the gaps. The evidence appears to me to be conclusive against Captain Tupman.

A. Brothers

Spectrum of the Aurora

There is one point in Mr. H. R. Procter's letter in Nature, vol. iii. p. 468, which I do not agree with. He says the bands of the auroral spectrum are seldom visible, with the exception of that whose wave-length is 557; whereas I have found two bands, doubtless Winlock's 464 and 431, to be invariably visible when the aurora is bright enough to show them. Also, I suspect the red line is always present when there is any red colour in an aurora, although our instruments do not show it unless the luminosity is considerable. Of the thirty-four auroras in whose spectra I have seen the line 557, fourteen showed the bands 46 and 431, and three others at least one of these, while eight showed the red line. In five auroras, all more or less red, I have seen a faint band, whose wave-length, I believe, is 500 or 510. I have never seen the line 532 (the coronal line), unless it be once; probably from want of instrumental power.

As regards the zodiacal light, I have looked at its spectrum several times when it has been at its brightest, but have never seen anything but a continuous spectrum. I am satisfied therefore that if the line 557 exists in it, it must be much fainter in proportion to the rest of its light than is the case with the aurora.

T. W. Backhouse

Sunderland, May 16

Science for Farmers

As the independent and powerful advocate of scientific education, will you allow me to draw your attention to the object of the enclosed letter?

Within a short period I have seen such remarkable results attended with such an enormous saving of money arising from a limited knowledge of science amongst a committee of farmers, that I am desirous the future generation should have at least a common sense idea of some of the laws of nature which more immediately concern their business and pecuniary interests.

I have the more faith in the success of what I am advocating, because the kind and amount of scientific instruction I propose is really a business necessity. I have not forgotten the results of the Great Exhibition of 1851, how the members of each particular profession or trade were interested, especially in such stalls or departments that concerned this main object of their lives, how to make their own calling more successful or profitable.

I believe therefore in the teaching of science a much greater prospect of success exists when it can be combined with a practical business pursuit. I have read with much interest your article "The Hope of France" on the paper read by M. Deville before the Academy of Science.

The advantages arising from scientific culture, in other words, the study of nature and her laws, are beyond appreciation, and for this reason a student of science must reason and think for himself; he must do his own thinking, and not allow any other person to do this duty for him; and it is my conviction that the real power of any State is exactly in proportion to the number of independent reasoners and thinkers that go to constitute it; and I know of no means so powerful to promote this as the extension of technical teaching applied to business pursuits.

May 18

W. Little

"TECHNICAL EDUCATION FOR THE SONS OF FARMERS

"*To the Members of the Lincolnshire Farmers' Association, and other Agricultural Associations in Great Britain*

"Gentlemen,—Will you allow me to ask your earnest consideration and reflection on a subject which I believe is of vital importance to the future generation of farmers? The question I would put to you is this: Do you wish your sons whom you destine to the pursuit of agriculture to be entirely ignorant of such of the simple elements of chemistry as would give them a complete knowledge of the application and properties of the various materials used in the manufacture of artificial manures, when such knowledge may be acquired with little trouble, in a short time, and at small expense?

"I cannot for a moment believe that any intelligent farmer, with the costly and bitter experience of the past few years in relation to the tricks and impostures of the artifical manure makers, can be so indifferent to the future success of his child as not to give him, by a brief course of practical scientific education, not only the power of protecting himself against fraud, but also the knowledge that will enable him to apply the gifts of science to the greatest possible advantage, and at the same time liberate himself from the large and plausible army of manure compounders.

"Why should the business and pursuit of agriculture be an exception in the rules of guidance for the successful pursuit of any other business or profession? For the practice of medicine, law, engineering, architecture, &c., a special course of study is required, and is really necessary. Agriculture as a business pursuit offers abundant occupation for the highest order of intelligence, and stands second to none in its claim to scientific skill and sound practical sense, and has therefore an equal claim with other professions, that those engaged in it should be properly qualified by a special form of education.

"What can be more embarrassing to the present generation of farmers than the reading of the reports of chemical analysts on the composition of soils and manures? What can they understand by 'water of combination' other than that it may have too near a relation to a pump; or the term 'organic matter,' which may mean flesh or bread, woody fibre, peat, sawdust, or coal dust, most likely a large proportion of the latter elements; or that very intelligible term 'soluble phosphate equal to bone earth or tribasic phosphate of lime made soluble;' or alumina, silica, alkaline, salts, &c., which generally mean clay, sand, and common salt, concluding with earthy matter as the dirty foundation upon which all the other perplexities stand.

"Is it not worth while, by a brief course of practical study, to rid one's self of the influence of all this chemical necromancy? The days of alchemy, witchcraft, and astrology have passed away, and so must the charlatanism and quackery of the inferior order of manure compounders.

"What would be the history of many of these occult persons if it could be traced? Should we find that they have at any time been diligent students under such guides as Liebig, Ville, Voelcker, and other honourable and distinguished chemists? No! I do not hesitate to say that many of them have been mere wandering vagabonds having no disposition or ability to get an honest living by ordinary industry, and as a last resource trade on the credulity of farmers as artificial manure makers. A case in point was recently reported to me. Two discharged lackeys, a butler and footman, embarked, for want of honest employment, in this trade. They are now millionaires; one is an M.P. and the other has received the honour of knighthood. Recently I was over the works of a large and respectable manufacturer of phosphatic manures, who was also a maker of sulphate of ammonia. He informed me that he mixed these two ingredients in such proportion that he could well afford to sell it for 6*l.* per ton. The mixture went in immense quantity to Liverpool, where it was christened under the name of phospho-guano, and was actually returned, more than a hundred miles, near to the original works, and sold for 12*l.* per ton. Are not such cases, and a thousand others, sufficient to make every farmer ask himself f one object of his being born into this world was to feed and fatten knaves?

"A first-class tailor, hatter, shoemaker, butcher, or baker, desires before all things that his customer should thoroughly understand the composition and quality of the goods he has for sale. Can the same be said of the manure compounders? The

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